# REVIEWS.

ART. XV. *Memoir of the Life and Medical Opinion of* JOHN ARMSTRONG, M. D. Formerly Physician to the Fever Institution of London; Author of "Practical Illustrations of Typhus and Scarlet Fever," &c. &c. *to which is added an Inquiry into the Facts connected with those forms of Fever attributed to Malaria, or Marsh Effluvium.* By FRANCIS BOOTT, M. D. Secretary of the Linnæan Society; Honorary Member of the Medical Society of Massachusetts. In two volumes. Vol. I. London, 1833.

THE circumstances of the life of JOHN ARMSTRONG, and the claim made for him to important improvements in the theory and cure of diseases, render a history of his life and opinions an object of no small interest to the profession. Neither the condition of his birth, nor the character of his early education, furnished indication of future distinction of any kind. We do not allude in this remark chiefly to the humble occupation of his father, as a glass manufacturer, for there are not wanting many examples of men who have risen from an origin much more unfavourable to an eminence far more elevated than ever Armstrong attained; nor to the fact, that at eight years old he could not read, and "was considered by his friends incapable of learning," for other stupid boys have turned out to be able and enlightened men. The unpromising circumstance in his case was, that he was ignorant and stupid, not for the want of opportunity, but for the want of inclination to learn. It was not that he had never been taught, that he was unable to read at eight years old, for he had been kept at school from an early age. His biographer attributes this ignorance to bad teaching; and bad indeed must have been the teaching that could not impart the power of mere reading in four or five years constant instruction; and he refers for proof to the fact that he afterwards learned with rapidity. In six months with a new master he could read distinctly.

"And," he adds, "after he found that he could learn, he studied the English language, Latin, Greek, and Mathematics: and having acquired a confidence in himself, his delight seemed to be to excel in every thing to which he turned his attention."

The truth seems to be that he was constitutionally indolent; with power of acquisition enough when he chose to apply it, but with little

disposition to make the application, except when stimulated to the exertion by strong and peculiar motives. At the age of sixteen he left Mr. Mason, at whose school he had made such progress, and was placed with a surgeon and apothecary. This situation, although he had shown an early inclination fur the medical profession, he soon left, contrary to the wishes of his parents, and remained at home, passing, it would seem, an idle, desultory life for two or three years.

We have dwelt the more on these particulars, because we think we perceive in them strong traces of this same character in his subsequent life. Whatever may have been his acquisitions during his eight years with Mr. Mason, he surely did not acquire a love of knowledge. He learned the English language well certainly, for we have few better or even so good writers in medical literature; his Latin and Greek have been disputed. He had strong powers undoubtedly, both of acquisition and of observation. But he wanted the industry necessary to cultivate the former, and hence he not only never became, even a moderately-learned man, but acquired a disregard for learning amounting almost to contempt. And in regard to the latter, his powers of observation, he required a strong stimulus of personal interest or ambition to induce him to exert them effectually. When he exerted himself rigorously, both in youth, and in his after life, he made rapid advances, and produced important results. But his efforts were inconstant and irregular; and therefore his early acquirements were extremely imperfect, and his later productinns incomplete. His great, not to say excessive confidence in his own powers, enabled him to meet any emergency into which he was thrown with little preparation but such as the occasion affurded; and thus he was led the more to disregard habits of steady application; consequently he postpuned and neglected much that he was extremely anxious to have performed; so that although he seems to have cherished with some fundness the belief that he had made highly important discoveries in medical science, which should confer distinction on his name in all succeeding generations, at the last, he left it to a friend to record, as well as to publish, the matured version of those opinions in which his discoveries were contained, and which were to perpetuate his fame.

At the age of eighteen or nineteen Dr. Armstrong entered as a medical student in the University of Edinburgh. Here he was precluded by the narruwness of his circumstances from extending his studies beyond the cuurse indispensably requisite to a medical education. As soon as he had obtained his medical degree, in 1807, he returned to Sunderland, his native town, and engaged in practice as a physi-

cian. The successful treatment of a wealthy and esteemed gentleman, to whom the friendship of a young university acquaintance had introduced him, gave him an early reputation, and established him at once in a valuable practice.

It was not until 1813, after he had been nearly six years in practice, that Dr. Armstrong appeared before the public as an author.

"His first publication," says our author, "was a short paper on the brain fever produced by intoxication, communicated to the Edinburgh Medical and Surgical Journal, then edited by the late learned and excellent Dr. Duncan, jr. It bears date November 13th, 1812, and was published in the number for January, 1813. This was followed by another paper on the same subject, in the succeeding number for April, in the same year, in which a case is fully detailed, with some very judicious observations on the nature of the malady, which in the example referred to, invariably arose in the collapse succeeding to the excitement of spirituous liquors," p. 11.

Dr. Boott adds—

"Having once ventured before the public as an author, he appears to have suddenly felt the ambition of arriving at distinction in his profession; for in this year, besides the papers alluded to, he contributed another to the same Journal, on cases of diseased cervical vertebræ; and in December he completed his 'facts and observations relative to the fever commonly called puerperal;' a work which was dedicated to his friend Dr. John Ramsay, of Newcastle, upon Tyne," p. 12.

Besides one or two other papers in the following year, he published in the same Journal in 1815—

"Brief Hints relative to the Improvement of the Pathology and Treatment of those Chronic Diseases usually termed Nervous," which, says Dr. B. "he thinks are generally secondary affections, inseparably connected with disordered circulation; and that if fixed principles as to their treatment are ever to be attained, it must be accomplished by extending our views beyond the nervous system to other textures; for that it will be found on examination they depend upon venous congestion or inflammation, and their consequences, or upon some disorder of function or structure in the viscera of the three great cavities."

We have noticed this series of publications chiefly as illustrating the course of observation by which Dr. Armstrong was led to the opinions which he formed, and soon after published in regard to typhus fever. We see him in each of these several diseases, instead of regarding the debility attending it as the essential feature of the disease, and the primary object of treatment, refer it to its true position as an effect of a preëxisting derangement, either of structure or function, which demands the chief consideration, both in the pathology and in the treatment of the disease. This is the leading cha-

racteristic of his work on typhus fever, which was first published in 1816, and which was the principal basis of his subsequent reputation.

"This admirable work," says our author, "at once raised him to a very high eminence in his profession. It passed through three large editions in three years, and was received almost with acclamation by the medical public, not only in this country, but throughout America, where it obtained for him, from some of the most eminent professional men, the name of the modern Sydenham."

Armstrong's "Practical Illustrations of Typhus," is a work so well known, that we need not follow our author into an examination of its character or its merits. Neither is it necessary for us to go into a very full examination of the justice or the extent of his claim to an important improvement of the pathology of febrile diseases. That Dr. Armstrong's views on this subject were in a great degree original in his own mind, there can be no doubt. Nor is there any more room to doubt that the general principle maintained by him, that the prostration and debility of typhus, as well as of many other diseases, are, to a great extent at least, the result of a previous excitement, has led to a most important improvement in the treatment of febrile diseases, as compared with that system which regarded debility as the chief object of attention and treatment. It is perhaps equally true, that to Armstrong much of the credit is due of bringing about so extensive a change in the opinions and practice of physicians. His bold and energetic style of writing is well adapted to produce a powerful impression.

It is no less true, however, that similar views had already taken possession of the minds of many others as well as of his. So great a revolution in the public sentiment of the profession could never have been brought about by the writings of a single individual without an extensive preparation in the opinions of a great portion of the profession, either already formed in its favour, or in a state of rapid progress towards it. Armstrong may have been the first, and it is no small praise to him that he was so, to embody these new views of disease into a distinct form, and to give expression to them, so as to exert, a powerful influence upon the pathology and treatment of fever, over a large portion of the civilized world; although many others may have imbibed similar views, in common with him, before they had learned them from his publications.

The success which Dr. Armstrong's work on Typhus met with, and the celebrity which it brought him, excited him to seek a more extended field of practice, and in 1818 he removed to London. To

those who have any adequate knowledge of the slowness and difficulty with which a stranger obtains extensive practice in a large and busy town, it will not appear strange that Dr. Armstrong's early success in London was less encouraging than he seems to have anticipated, or that he had overrated the effect upon his professional prospects which he expected from the popularity of his writings. The influence of successful professional publications upon the public at large, is doubtless much greater in England than it is in our community. But there this influence if left to itself must have been much too slow in its operation, for the necessities of a man who had to rely upon the practice he was to obtain to support a family already consisting of a wife and two children. That Dr. Armstrong succeeded at all, so as to sustain himself in London, and ultimately to arrive at distinction there, seems to us to have been owing, in a great degree, to the very circumstance which, in its first occurrence, appeared the most to threaten to be the cause of his entire failure. We allude to his rejection by the examiners of the College of Physicians. We give Dr. Boott's account of this event in full.

"In the spring or summer of 1818, Dr. Armstrong presented himself for examination at the London College of Physicians, conformably to its regulations, which require that the graduate in medicine of any other University than Oxford or Cambridge, should pass the ordeal of its favour, and obtain its license before entering upon practice in London, or within a given distance of the metropolis. He had, perhaps, undervalued the estimate which the board of examiners place on classical diction, and the alphabet of the profession; for this distinguished physician, who had received a diploma from the most efficient and most celebrated school of medicine in Great Britain—who had been in successful practice eleven years, and was the author of three of the most popular works which the medical press of this country had ever put forth, the fame of which was still sounding in the periodical journals of the day, was rejected as incompetent to continue in the practice of his profession in London, and as undeserving the honour of having his name enrolled among the members of the college."

"This public stigma," he continues, "of the justice and motives of which I leave to others to judge, was not without its natural and perhaps salutary effects upon the sensitive mind of Dr. Armstrong. His nature was mild, but too dignified to submit to insult and unmerited wrong, which threatened injury to his own reputation, and ruin to the welfare of his family. He did not admit the necessity of any particular attention to his profession to qualify him for passing the usual examination the next year, as he was aware that the first rejection was generally the only one. But he felt roused to the due assertion of his own claims to respect; and from the impressions which this act of wanton power made upon him, are to be ascribed much of that indignant tone which afterwards sounded in his lectures on scholastic institutions," p. 29.

We feel little interest in the controversy which arose between Dr.

Armstrong and the College of Physicians, and which has been repeatedly enacted and reënacted for the last hundred years, by successive claimants to some of the exclusive privileges of that privileged body. But the duties of boards of medical examiners are of so much importance, both to the public at large and to the profession, that we can hardly pass this transaction by without a few remarks. We do not perceive all the justice of the severe strictures upon the result of Dr. Armstrong's examination, as "an act of wanton power." If it be conceded that it was right that he should submit to be examined at all, it was indispensably necessary that the result should be determined by the extent of qualifications exhibited in that examination. It may be a question how far a board of examiners may be authorized to offset peculiar excellencies in one department of professional knowledge against obvious deficiencies in others. This is a discretionary power which always causes great difficulty and embarrassment whenever the exercise of it is called for; and in regard to which there are certain points beyond which it ought not to go. There are some things which every professional man ought to know; and if need be, he should be *required* to know them before he shall be received into the pale of the profession, and no degree of excellence in other particulars should authorize any board to dispense with them. To what extent Dr. Armstrong was really deficient in these "necessary things," we are not fully informed. We have, indeed, heard that he was deficient in the knowledge of some things which every man of science, and especially every professional man, is expected to know; and Dr. Boott hints at a failure in "classical diction," and in the "alphabet of the profession." We are not quite sure that we receive the right meaning of these terms. But if the meaning they are designed to give be that which they naturally convey to our own minds, we feel bound to say that in our opinion, Dr. Armstrong, as a man of science, as a member of a learned profession, still more as a teacher of medicine, had been much better employed in acquiring that "diction" and learning that "alphabet," than in "sounding his indignant tone on scholastic institutions."

The question still remains, however, whether the College of Physicians had any just right to require of Dr. Armstrong to undergo an examination before their board of examiners. He had already received a degree, and of course had undergone an examination at Edinburgh. Why should another examination be required of him? It must here be remembered that there was nothing personal in this requisition. The requirement is an universal rule; a dispensation from, rather than the enforcement of, which would have been an act of ar-

bitrary, if not "wanton" power. We must remark, too. that the authority which requires every practitioner of medicine in London and its environs, to be licensed by the College of Physicians, is not, as Dr. B. seems to suppose, one of the regulations adopted by the college, but is contained in the charter of the college, and is therefore a part of the law of the land. The College of Physicians have indeed adopted a by-law, and have pertinaciously adhered to it through every degree of opposition, which pruvides that no physician shall be elected a fellow, who has not taken his degree of doctor of medicine at one of the universities of Oxford or Cambridge. A more absurd rule cannot well be conceived of, nor one calculated to operate more injuriously on the character and condition of the medical profession within the sphere of its action. Neither of those universities furnishes facilities for a thorough medical education at all to be compared with such as are supplied by many other schools, both on the continent and in Great Britain. The by-law is therefore equivalent to an enactment that no man shall be eligible to the dignity and privileges of a fellow of the College of Physicians who has not contented himself with means and opportunities for professional acquirements decidedly inferior to what were equally within his reach.

This by-law does nut apply at all to licentiates of the college, and had therefore no direct bearing upon the case of Dr. Armstrong, in regard to whom the question could not have been of admission as a fellow, but simply of license to practise medicine, as a person "profound, sad and discreet, gravedly learned, and deeply studied in physicke," (as the charter of the college expresses it.) Still the great body of licentiates have none the less reason to complain of its oppressive and unjust operation. For it not only cuts them off from all access tu privileges, which ought either to be distributed equally to all, or, at least, open to honourable competition, and confers those same privileges arbitrarily upon men who are almost necessarily less deserving of them than themselves; but since the power of examination is exclusively in the fellows of the college, it subjects the licentiates to the scrutiny of men whom, by the same necessity, they can hardly fail to regard as their inferiurs in real merit, notwithstanding their nominal superiority in title and privileges.

The general good of society, as well as the prosperity and respectability of the medical profession, demand that their should be in every community a competent tribunal, to decide upon the qualifications of candidates for medical practice; to distinguish, as the charter of the College of Physicians, granted by Henry VIII. expresses it, the uncunninge from the cunninge. Nor is it in all cases enough that the

candidate should undergo such a scrutiny only at his first entering into practice. If a physician remove beyond the authority of the body which first took cognizance of his qualifications, it is but reasonable that the community to which he goes should be furnished with the evidence of his fitness to be received into their fellowship. This need not always require a second examination. If the candidate present evidence of having passed a sufficient examination before a competent tribunal, such evidence may be received as an equivalent. This, however, is rather a matter of expediency in the examining tribunal, or of courtesy to other similar bodies, than of right to the candidate who presents himself; and there are cases to which it cannot apply.

Although it is true in the abstract, that the value of human life and health is the same in all places, and therefore that the qualifications of physicians should every where be the same, yet in actual practice it is far otherwise. That community which offers the most liberal remuneration to an enlightened and learned body of physicians, will demand and obtain men of higher attainments, than another where the rewards of the profession are of less consideration. The standard of qualifications for admission into the profession, must therefore be as different as the different states and wants of society. And it becomes the duty of each examining tribunal to judge of those qualifications, not merely as an abstract question, but chiefly in reference to the condition of the profession in the community to which they belong. This principle has been, and probably always will be regarded with some degree of jealousy by those upon whom it chiefly operates, and its execution has not unfrequently drawn a great deal of odium upon those who insist upon enforcing it. The man who goes from some interior or provincial institution, where he has obtained his professional education at half the cost, if not with half the opportunities, of those among whom he proposes to establish himself, takes offence that the testimonials he carries with him are not received as full evidence of his competency to share equally in their privileges; and the institution from which he proceeds unites perhaps in his indignation, and resents the rejection as an act of disrespect to itself. Yet both are unreasonable. Those to whom he would join himself have passed a severe scrutiny themselves; and they owe it to the community, which affords them a liberal remuneration, to sustain the full elevation of professional attainments.

Nor is there any serious difficulty in the practical application of the principle, complicated as it may seem, in the abstract to be. The statutes of each institution which fix the standard of preliminary education are published; and the conduct of each under those statutes

is generally matter of public notoriety. When a candidate presents himself to an examining tribunal with credentials from another, if the institution from which he proceeds has adopted a standard of qualifications as elevated as its own, and in practice honestly adheres to that standard, it is fitting that he should be received upon the faith of his testimonials. But if, on the contrary, he comes from an institution which judges a less amount of attainment to be sufficient, or which is accustomed, while it fulfils the requirement to the eye neglects it to the sense, (and such medical institutions there are,) it is not only reasonable, but absolutely necessary for the security of the public, and the character of the profession, that he should furnish further evidence of his attainments.

It is in this manner that the qualifications of candidates are attested by some of the best-established medical societies in this country; and although a little jealousy has sometimes been felt and expressed by some of the minor institutions, the system has been found on the whole, to secure pretty effectually the community from the inroad of ignorant pretenders, and the profession from open dissention, and in a great degree from private discord. There is probably no portion of the globe in which the medical profession exhibits so much harmony among its members, and enjoys so extensively the confidence of the community as in some parts of America. If such be the fact, and we feel quite confident that we are not mistaken, it is owing chiefly to the character and administration of our public medical institutiuns that it is so.

But to accomplish these objects, it is essential that the medical institutions themselves should be properly constituted. It is sufficiently obvious, from the very nature of the case, that in any community the medical profession are the proper judges of the degree of qualifications which that community demand in its physicians. But this power properly belongs to the profession as a whole, and not to any detached portion of it. The *whole* profession, therefore, acting either collectively, or by authorized delegation, should fix the terms of admission to their fraternity; and entrust the execution of their rules to men, selected by, and responsible to themselves. In this way alone do we conceive that the decisions of the examining tribunal will be sure to receive the sanction of the profession or the confidence of the public. And without these they are of little value. In these days, the power of mere law, or of exclusive privileges, does little to give efficacy to the judgment of a board of examiners, when that judgment is opposed to the sentiment of the more respectable part of the profession. This is a point of some importance, and

worthy of more consideration than it has received. We cannot now examine it fully, but must be content with remarking that the whole substantial value of the decision of any examining tribunal, depends upon the degree of confidence placed in that decision; and the confidence of the public at large, is measured almost exclusively by that of the profession. A tribunal which does not possess this confidence, may do much to embarrass and perplex the course of those whom it may wish to discountenance, and much to disturb the peace of the profession; but it cannot control the conduct of individuals, nor secure the harmony of the whole.

In this particular our medical institutions differ most essentially from those of England. It is true, indeed, in our own country, that the greater part of our schools for medical education are not under the immediate supervision of the profession, and some of them do not recognise any *direct* influence from it. It is equally true, however, that the medical societies in each state, expressing as they do, the collective voice of the whole profession, have by their organization a controlling power over the education of candidates, which no medical school in our country could effectually resist. Happily collision is not likely to arise between the two classes of institutions, for the interests of both are pledged to maintain a high and healthful standard of professional attainments. But if any medical school were to become so unmindful of its duty, and true interests as to attempt essentially to lower the standard, it would find an effectual check in the medical societies by which it is surrounded.

How different all this is in England, is shown by the constant contention between the college of physicians, and many active and highly respectable members of the profession, and most emphatically by the case of Dr. Armstrong. We have already seen that the London college of physicians is neither made up of the whole body of educated physicians, nor of a delegation, nor a selection from them; but is a privileged body selected by an arbitrary rule from a limited portion, and this portion not likely from the nature of the case, to be made up of the best educated, or most able members of the profession. It needs no evidence to show that a body thus constituted can never possess the confidence of the profession as a whole. If such evidence were needful, the whole history of the college of physicians for more than a hundred years furnishes it most abundantly. This same history would furnish equal proof of our other position, that without the sustaining influence of the profession, the decisions of the college will not be conclusive with the public at large. But we confine ourselves to the case of Dr. Armstrong.

We have seen that Dr. Armstrong, a stranger in London, except so far as his publications had made him known, was examined before the censors of the college of physicians, and was rejected. Such a rejection from a tribunal properly constituted, would not assuredly have been final. It was quite otherwise with him in the present case. Whether indeed Dr. Armstrong renewed his application the following year, and was licensed, or whether he went through his subsequently prosperous life in despite of all the powers of the college of physicians for fine and imprisonment, Dr. Boott by a singular want of explicitness in his account of this transaction already quoted, has failed to inform us. In either case the rejection instead of retarding his professional advancement, evidently hastened it, by the strong interest it excited in his favour. Soon after it happened, Dr. Armstrong was elected as successor to the late distinguished Dr. Bateman, one of the physicians to the London Fever Hospital. The circumstances of his election, which seems to have had a very decisive influence upon his subsequent prospects are thus related:—

"During his residence in the north of England, Dr. Armstrong had practised extensively among the society of friends, and he owed to the recommendations of some of its members the distinguished favour which he met with from the board of trustees of the fever hospital. There was one gentlemen, I believe, of this religious persuasion, who, though personally unknown to Dr. Armstrong, particularly favoured his claims. His appointment as physician to the hospital was made known to him on his introduction to the trustees, he expressed his grateful sense of their kindness, but at the same time regretted he could not avail himself of it, as their laws required that their physician should be a fellow or licentiate of the college of physicians, and that he was not a member of that body. On their inquiring the cause, he told them that he had offered himself for examination as a licentiate, and been rejected; that no cause had been expressly assigned, but that the sufficient one undoubtedly was, the opinion of the board of examiners of his incompetency to practice his profession. On his announcing this fact he was desired to withdraw, and the gentleman above alluded to, in his absence, spoke of the estimation in which he had been held in the north of England, of the high reputation which he had obtained from his writings, and it was proposed that the by-law, which made it necessary for their physician to be a member of the college should be immediately suspended. This was assented to, and Dr. Armstrong entered at once upon the duties of the important office thus honourably conferred upon him," p. 31.

We do not believe there is a medical institution in this country, which possesses the confidence of the community around it so little, that it would be possible to procure a reversal of its decision in a case like this. And we regard this as evidence of the most incontrovertible kind, of the low estimation in which the London college of

physicians is held by many of the more enlightened and intelligent people of that metropolis.

From the time of his appointment to the Fever Hospital, Dr. Armstrong's practice increased rapidly, and he soon found himself firmly established in London. We need not therefore pursue his biography further in detail. The life of a professional man, after the obstacles to his first establishment are surmounted, like the tale of romance, when the impediments from the course of "true love" have vanished, furnishes little of striking incident to fill out a narrative. It was certainly highly creditable to Dr. Armstrong, that he owed much of his prosperity and celebrity to the favourable regard of his brethren in the profession. There was probably not a little of party feeling intermixed with the personal attachment. A man who had suffered so publicly, and triumphed so signally, was by the very nature of his situation pointed out as a leader in the opposition to oppressive "scholastic institutions."

If it be true, however, that the circumstances in which he was placed did much for his prosperity and reputation, it must not be forgotten, on the other hand, that his own character and conduct formed, in a great degree, those very circumstances, and carried him successfully through them.

But the principal cause of his early and rapid success in London seems to have been the charm of his personal character. His energy, and even enthusiasm, when roused, as he now was, to strenuous exertion, his clearness of mind, especially on subjects to which he had given peculiar attention, his amiable deportment and warmth of feeling won for him the respect and confidence of those members of the profession who came into contact with him, and the esteem and ardent affection of many friends.

"He owed his success in London," says Dr. B., "to two causes, for no one had ever fewer adventitious aids to success, and the one reflected as much honour upon his talents as the other did upon his disposition. Those members of the general profession who had once experienced the benefit of his counsel and assistance, could seldom be induced to recommend any other physician, so strongly impressed were they with the simplicity, the originality, and success of his views and practice; and those families who had once had an opportunity of feeling the effects of the gentleness and delicacy of his manner, could think of no other adviser. There are many persons in and out of the profession who will admit the truth of these remarks, and who will confess that the loss of this eminent man appeared to them irreparable. He had the faculty of communicating his ideas to others, in the most easy and intelligible manner, and from the fertile resources of his own mind, of throwing light upon the most ob-

scure and involved cases. Those difficulties which embarrassed common minds, seemed at once charmed away by the magic influence of his own; and his opinions were delivered with so much candour and perspicuity, that while others bowed before the superiority of his intelligence, they were instinctively impelled to place the fullest confidence in his skill and integrity, and to feel an irresistible affection for his character as the man blending with their admiration of his talents as the physician. His manners were simple almost to a fault, and were at first forbidding, from the absence of every thing like an attempt at effect; but no sooner did he enter upon the consideration of a case, than it was apparent he was completely absorbed by it. His seeming reserve at once gave way to a visible feeling of deep and tender interest in the welfare of his patient, who felt satisfied that he was in the hands of an amiable and sagacious man, to whom he might confidently entrust himself," p. 33.

If it should be thought there may be something of the ardour and partiality of friendship in the warmth of commendation in this and similar passages, it nevertheless scarcely the less faithfully exhibits the amiable traits of Dr. Armstrong's character. For those qualities of mind and heart must have been of no ordinary cast that should have excited so distinguished, so ardent, and so permanent a feeling of admiration and affection in such a man. Indeed this whole work may be regarded as a tribute to the high moral qualities, not less than to the intellectual attainments of Dr. Armstrong, and the author in setting them forth with such warmth of feeling, has unconsciously presented us with a most pleasing exhibition of his own character, while he has taken nothing from our confidence in the faithfulness of the picture he has drawn of his friend.

In the year 1821 Dr. Armstrong became a public teacher of medicine, in the Webb street school, established by himself in conjunction with Mr. Grainger. The celebrity which this school for several years enjoyed, must have arisen mainly from Dr. Armstrong's ability and popularity as a lecturer, especially after the death of Mr. Grainger, which happened in two or three years. Indeed the labour as well as the reputation of the school seems to have rested mainly upon him, for we find him soon after, besides his course on the Theory and Practice, giving also a course on the Materia Medica. And since his death, the school, we believe, has been abandoned.

Dr. Armstrong's enterprise was not diminished with his prosperity. He not only carried on his various lectures, and attended to his increased practice, but made several publications which our limits do not allow us to notice particularly. His ambition, stimulated by the course of distinction and fame which he saw opening before him, urged him to the utmost exertion of his powers. Unfortunately, however, both for his usefulness and for the perpetuity of his fame, his

energy was stimulated more to the projection and commencement of new works, than to the completion of those already begun. Much of this is doubtless to be attributed to the early period at which, in the midst of his activity and enterprise, all was arrested by disease and death, but more we think to the peculiar tendency of his character and early habits.

Dr. Boott remarks of the early education of Dr. Armstrong—

"However limited his opportunities of improvement had been, I cannot but consider that his education was eminently calculated to bring forth the original powers of his mind," p. 7.

To us, this whole matter appears far otherwise. True genius is not so bound down by the circumstances of education; while on the other hand, the efforts of the ablest minds are not unfrequently, for the want of a proper education, rendered abortive or unproductive. Such in our view was the case, to a great degree, with Dr. Armstrong; and that it has not proved so to a much greater extent, is owing, we believe, quite as much to the labours of his able and zealous friend, as to the character of his own productions. Whatever may have been the value of the several works published by himself, or the importance of some of the views, both practical and theoretical, which he either introduced or advocated, there is about them all, a want of maturity and thorough consideration of the whole subject treated, which we apprehend will prevent their sustaining a high rank as permanent standard works, and which will therefore deprive him of much of the lasting fame which he confidently anticipated.

Of the imperfections of his earlier publications, Dr. Armstrong was himself deeply sensible at a later period of his life, and "regretted that his life could not be prolonged to enable him to republish them in a more satisfactory form;" and so unwilling was he to rest his future reputation upon them, that he left it in charge to his friend to make known to the world the results of his more mature observations and reflections. His opinions on his favourite subject of fever, underwent some very important changes, subsequently to the publication, even of the latest editions of his several works on the different kinds of fever. Dr. Boott attributes these changes of opinion to more extended opportunities for observation; and he claims for his friend a no small meed of praise for his candour in being willing thus to change, when these opportunities were presented to him. But other men had observed before, if Dr. Armstrong had not; and a true philosophy would have taught him, to make use of the observations of others as well as his own. To imbibe the *opinions* of others without examination, or without sufficient evidence, may be, and

often is, a proof of imbecility. But to learn from the facts observed by others, argues any thing other than servility of mind. On the contrary, it is one of the truest marks of original, inventive genius, to distinguish between the observations and the theories of others, faithfully to apply their facts, and to receive or reject their speculations, according to the evidence which supports them; to range over the whole field of knowledge, and search out observed truths, however buried amid speculation and hypothesis.

The man who limits his knowledge to what he can himself see, necessarily limits himself to a corner. And this was the fault of Dr. Armstrong's mind. With him, seeing only was believing; and he would consent even to *see* with no eyes but his own. When he changes his views, it is not because he finds that those views are not supported by the phenomena of disease as observed on a large scale throughout the world—it is because what he sees in London is inconsistent with what he saw, or thought in Sunderland. London is a large place, doubtless, and the London Fever Hospital is an excellent institution, and affords a most favourable opportunity to observe disease. But the world is larger, and other men have seen fever, and described its phenomena, besides Dr. Armstrong.

The course which Dr. Armstrong took himself, in regard to his medical opinions, he taught his pupils to take.

"The question," he says, in his lecture on typhus, "whether typhus fever is contagious or is not contagious, cannot be decided by any reference to black-lettered books; but by a reference to facts, and facts alone, contemplated with the most perfect impartiality."

We are not to suppose that Dr. A. used language so feebly as to mean no more than that the question of contagion is a question of evidence, and not of theory merely. There is an obvious antithesis, such as we find in his whole life and character, between learning derived from books on the one hand, and personal observation on the other. This may be pleasant hearing to many a pupil to whom the "black-lettered book" is a sad eye-sore, while he pleases himself with the fancy that he is gaining experience, and learning how to treat disease merely by seeing it. This outcry against books always reminds us of a reply of a disciple of a late distinguished *rubbing* empirick, to a friend of ours, who had asked him, "where his master obtained his peculiar knowledge—from books?" "No, not from books," said he. "And why not?" "It is not there." "What! none of it," said our friend, "among so many books?" "No, none," said the disciple. "True," he added, "there was another reason; *he could not read them.*" We know of how little practical value is

mere learning, without personal observation. But we must also say, and we would fain sound it in the ears of every medical student, that no extent of observation or experience, without study, diligent study, will ever make either a sound philosopher, or a good physician.

Dr. Armstrong's course in London was, from the circumstances in which he was placed, a highly exciting one, and it was on the whole, brilliantly successful. But it was short. In a little more than ten years from his removal to London, the disease, which was to prove fatal, fixed upon him; and after another year of most painful struggle between the claims of professional duties and the advance of disease, he was carried off by consumption in December, 1829, in the forty-sixth year of his age. There is a moral lesson in all this, which will not fail to force itself upon our thoughts and feelings, even in the midst of professional discussion. If the life of man be all contained in this mortal part, then let it be occupied and absorbed in agitation and excitement. It is short and full of disappointment; but if it be all, it were wise to make the most of its pursuits and enjoyments. But if it be but the entrance to another, as lasting and as full of high capabilities, as this is evanescent and unsatisfying, then surely there are better objects of pursuit, and more elevated motives of action, than the attainments of scientific research, or the highest aim of professional ambition.

Much the greater part of this first volume, as is to be the whole of the second volume, is occupied with an examination and elucidation of Dr. Armstrong's opinions on the subject of fever. We have seen that this subject engaged his attention at an early period of his professional life; that his publications upon it first gave him celebrity, and encouraged him to pursue his fortunes in London; and that there his appointment to the Fever Hospital insured his success in that metropolis. We might have added, and perhaps in justice to him we ought to have done so, that his lectures on fever attracted a very large share of attention from professional men, as well as from pupils. To his own mind, his views of fever had all the charm of original discovery, and all the vividness and much of the clearness of established truth; and he seems to have had the power of imparting in a remarkable degree these impressions to his auditors.

It appears to have taken nothing either from his own or his hearer's confidence in the correctness of those views, that they underwent some very considerable changes in the course of his investigations. The modesty of the learner combined with the intelligence and confidence of the teacher, presents too interesting a specimen of can-

dour to allow the intrusion of very scrupulous doubts whether further learning may not still further change the view of what now seems sufficiently clear and established. To our minds, cooled by the intervention of a few years time, and the distance of a few thousand miles, the suggestion presents itself, that since the enlargement of his sphere of observation from Sunderland to London caused some important modifications of his opinions, a further extension of his view to the diseases of the world might possibly have produced changes of opinion even more considerable and important. Of this, however, neither he nor his pupils had any apprehensions. What he saw, or thought he saw, he saw vividly, and he described it with a clearness and reality which powerfully impressed the minds of his hearers.

Of all his friends no one appears to have entered more fully into his views of disease than Dr. Boott; and no one surely could have described them more perfectly or defended them more ably than he has done. Indeed, we greatly doubt if Dr. Armstrong had himself accomplished the revision of his opinions, whether he would have produced so satisfactory a work—whether he would have stated those opinions with so much clearness and precision, and supported them with so much research and ability, as has been done by the friend to whom in his dying moments the task was committed. The charge of perpetuating his fame could not have been placed in better keeping, whether we regard our author's affection for his deceased friend, his zeal for his reputation, his respect for his talents and character, his confidence in the truth of his opinions, or the ability with which he has executed his interesting and responsible trust.

For the soundness of the opinions themselves, ably elucidated as they are, we are not indeed prepared in all respects to answer. Fever in all its varieties is so complicated a disease, involving so great a variety of phenomena, and attended with so many changes of structure and function in the animal system, that we have little expectation of ever seeing the whole satisfactorily explained, by any one theory applicable to all its different forms. Almost no two systematic writers on fever will agree as to the characteristic features of the disease. Each includes just so much among the essential characters, as his theory most readily explains, and rejects as accidental and adventitious, whatever of the ever-varying phenomena, will not easily fall into his rank and file.

Dr. Armstrong's theory of fever is comprehensive and simple; and it wants but a full conviction of its truth to regard it with admiration.

"One of the most striking characters of his mind," says Dr. Boott, "was a

power of generalization, which enabled him to grasp at once a complicated subject, and to view it from an intellectual elevation unattainable by men of ordinary powers."

But from this elevation might he not overlook some peculiarity in the phenomena or their causes, which must necessarily arise in the diseases of the living system in all the varieties of climate and condition in which the human race are found? In the natural sciences a power of generalization has raised some of the masters of science to the highest distinction; and we do not wonder that a man who has so successfully cultivated those sciences, as has our author, should have carried with him into the medical profession a peculiar respect for such a power. But in the investigation of disease, there are so many circumstances of climate and season, and of the condition and constitution of the patient, to give a peculiarity, and a sort of individuality to each particular case, that an extended series of cases can hardly be *generalized* into any system, without taking for granted as a matter of inference, many things which should only be received on the fullest observation.

A very large proportion of diseases have certain points of resemblance; yet these resemblances furnish little evidence of a uniformity of character, and afford no indication that they require similar treatment. The several varieties of fever have perhaps as much that is common in their history and symptoms, as there is between the whole race of quadrupeds in natural history. And he who would thence derive for them a common origin and prescribe a common treatment, would be as skilful a physician as he would be a naturalist, who should assign similar habits and powers to the animals that run on four legs. Were it possible to go further, and as in natural history the habits of animals may be traced to their intimate organization, to trace the phenomena of diseases back to known causes, then indeed we might generalize in medicine as well as in the other sciences. But here, we contend, the analogy ceases. From phenomena observed only in reference to comparatively a few cases, we are obliged to infer both cause and effect in reference to a great many others; and others too, in regard to which there may often be circumstances operating of which we have no knowledge.

We do not mean to say that Dr. Armstrong's generalization is altogether so sweeping as that we have just supposed. Yet when we see how easily he disposes of all the various forms of fever, including many diseases in which the febrile character has generally been regarded as subordinate, it seems to us truly that his system runs somewhat upon all fours.

Our limits will not permit us to give more than a very brief summary of Dr. Boott's able and clear account of Dr. Armstrong's views of fever, those chiefly which he entertained at the close of his life. He divided fever into two great classes, common and specific, according as the cause which produces it is either *common* or *specific.*

"The common causes are those ordinary agents to which mankind in every situation and under all circumstances are exposed. These are classed according to their primary effects; their ultimate effects upon the body when these amount to a powerful impression, disturbing the functions and affecting the structure, constituting what he denominated common fever; which not only included the inflammatory affections comprised in the phlegmasiæ of Dr. Cullen, but diseases of a similar nature, scattered in different parts of his artifical arrangement. It also comprised other disorders not inflammatory, the symptoms and pathology of which, by a legitimate generalization, were shown to depend upon the different stages or degrees of action arising from the exciting cause of fever," p. 115.

The common causes he divided into three kinds—common depressants, those common agents which enfeeble the energy of the vital powers, and diminish the animal heat, the action of the heart, and of the muscular system, such as low and variable temperature, severe accidents, or surgical operations, &c. Common stimulants, which affect the whole body, augmenting the energy of the vital powers, increasing the animal heat, &c., such as high temperature, great bodily exertion, or powerful exciting emotions of mind, &c.; common irritants, which primarily affect particular parts of the body, increasing their sensibility, but ultimately operate through the local impression upon the whole system, acting as depressants or stimulants according to the condition of the patient or other circumstances. These are fermented liquors, indigested food, &c. The fever produced by the first of these causes has three stages, that of oppression, excitement, and collapse; that by the second cause, has only the two last stages of excitement and collapse.

"The specific causes of fever are those occasional agents to which mankind are not universally or equally exposed. They operate generally as depressants, and differ from common causes in invariably giving rise to fixed and peculiar, as well as occasionally to variable and common effects; the particular cause itself always operating on particular structures, probably through the blood, and thus giving rise to a fever of a peculiar or specific kind." "These causes are certain states of the atmosphere, specific contagions, and poisons," p. 117.

Each of these causes gives rise to fever under three forms, congestive, simple, and inflammatory; so that we have, as embracing all the varieties of fever, common congestive fever, common simple fe-

ver, and common inflammatory fever; and specific congestive fever, specific simple fever, and specific inflammatory fever.

Neither our limits nor our inclination suffer us to attempt an extended examination of this view of fever. We have recently gone over much of the same ground,* and we have not the heart so soon to retrace our steps. Besides, we are not very confident that the utility of these discussions are an adequate compensation for the labour they require, unless it be by the warning they give, by showing how easily the best powers of observation are led astray when once the love of theorizing has taken possession of the mind.

The mere statement of this view of fever, shows how entirely hypothetical it is in all its parts: and yet Dr. Armstrong seems to have regarded it all as a simple deduction from his own observations; and our author so far partakes of the same impression that he no where calls it even a theory, but speaks of it as a description of what takes place in the production and course of the disease. The cause of disease is often unknown, or at best is known only by a remote influence; and its mode of action is still less understood. By making these the basis of the whole system, instead of being guided by the phenomena of the disease, diseases are brought together which have little affinity, either in their symptoms, or the treatment they require, while others are separated which cannot be distinguished by any thing which occurs in the course of the disease itself. Thus, intermittent fever, remittent fever, including yellow fever and the plague, and typhus, are one and the same disease, differing only in degree; while yellow fever is regarded as three distinct diseases, which almost all the writers on that disease have confounded together, and two of which it is conceded, one common fever, the other specific, cannot be distinguished by their symptoms, and require no essential difference of treatment.

"That the different forms of typhus," says our author, still giving Dr. Armstrong's view of fever, "are merely degrees of one and the same disease, appears by their passing, in some cases, from the milder to the severe modifications, or re-passing from the severer to the milder; thus, intermittent fever sometimes becomes remittent, and this continued; or the continued becomes remittent, and this intermittent."

But what if it should turn out that whole districts of country are subject to one form of fever, while for a long series of years the others are never found there? This is precisely true in New England. Nearly the whole of New England is perfectly free from intermittent

* See Review of Smith and Tweedie on Fever.

fever, excepting the few cases of those who have been exposed to its cause elsewhere, and has been so for a long period of years. In the early settlement of the country, this portion of America suffered as other new countries almost always have done from intermittent and remittent fever, as well as from other diseases; and at later periods, the overflowing of a meadow by the erection of a dam, or some similar operation, has occasionally given rise to it within a very limited space. But with the exception of cases of this sort, in which the disease has always disappeared whenever the local cause has been removed, we are not aware that a case of intermittent fever has originated in New England for many, probably more than a hundred, years. Typhus fever, on the contrary, or, to conform to the language of Dr. Armstrong, continued typhus, is to be found in every part of New England almost every year. It rarely becomes epidemic; but sporadic cases occur annually, both in the towns and throughout the country.

The explanation of the pathological distinction between these different forms of fever, is still more remarkable.

"The distinguishing characteristic symptoms of each merely depend on a difference in the degree of the pathological effects produced. In the mildest form there is no apparent inflammation; in the severer form there is inflammation; and in the worst form this inflammation is so much more extensive or intense, as to prevent any intermission or remission of the symptoms," p. 174.

Again—

"The first [intermittent] is a simple fever, or one of simple excitement; and the two last [remittent and continued fever] are inflammatory. The inflammation in both attacks the same structures; but in the remittent form it is not so influential as to prevent more or less a morning remission of the symptoms, while in the continued form it is so urgent as to make the fever assume a continued type," p. 174.

We know not upon what evidence it is assumed that intermittent fever is unaccompanied by inflammation, nor are we told what is the nature of those visceral enlargements which so often accompany this form of fever as to be familiarly denominated ague-cake. Neither are we presented with the evidence that inflammation always attends remittent or continued fever. It may perhaps prove nothing more than our want of sagacity to detect inflammation, to say that we have seen cases of typhus in which we were unable to discover it; but we may say that cases are recorded by able and acute pathologists in which no symptoms of inflammation were observed during life, and no marks of it could be found after death.

But we gladly turn from this subject to meet our author on the fair

field of actual observation: and here we are so well pleased with his labours that we could fain have wished he had never been induced to leave it even to follow with such eminent ability his distinguished friend in his speculations.

Dr. Armstrong's opinions in regard to the cause of typhus underwent an entire change in the course of his life. At the time of the publication of his volume on the disease, he regarded contagion as the sole cause of the disease. Afterwards he doubted, and at the last, as we have seen, he considered it as interchangeable with intermittent and remittent fever, and like them produced exclusively by malaria. Dr. Boott has enlarged upon this last opinion, and gone into an able and extended examination of various epidemics in reference to the question of their origin or their mode of propagation. This examination occupies the largest, and in our view, much the most valuable part of the volume before us; although he has so far only examined the fevers of this country. In another volume he promises to subject to a similar scrutiny the fevers of Europe from the time of Sydenham.

In his examination of the fevers of America, although Dr. Boott considers them, in some measure, in reference to Dr. Armstrong's peculiar views of fever in general, yet we do not perceive that those views deter him at all from a faithful consideration of their true character as exhibited in the best descriptions of them to which he had access; for example, he follows Dr. A. in regarding all our fevers as modifications, or different forms, of typhus; but when he inquires into their character, he investigates them as so many independent diseases, with very little reference to their supposed relation to typhus. He has given us therefore an able essay upon the origin of several of the most important epidemic fevers of this country; in which he brings together abundant evidence of their non-contagious character. The discussion of this subject has indeed less of interest and novelty attached to it in this country than it appears to have in England; because it has now become so fully settled that few among our physicians we believe have any doubt in regard to it. Still this volume will be found to possess a high degree of value to American physicians. For we know not where else they will be able to find so clear and satisfactory a view, in so condensed a form, of the proof of the non-contagious character of the diseases of which it treats.

It is curious to see how little Dr. Armstrong, in the change of his opinions on the subject of contagion, acknowledges any influence upon his own mind of the opinions or observations of others; if he was indeed really aware that such observations existed. We have seen that when he published his volume on typhus, he fully believed in its

contagious character. This opinion he still held when he removed to London in 1818. In his published lecture on typhus, he says—

"It is a singular circumstance, that when I first settled in London the current opinion among the profession was, that typhus fever originated *solely* in human contagion; and it is remarkable that it should have been reserved for me to *discover* that mistake in this metropolis," p. 163.

In 1822 Dr. B. tells us his opinions were wavering.

"In 1825, and especially towards the close of his life, the doubts which he had entertained on the subject were almost entirely removed; and he confidently anticipated the time when the same change which had occurred throughout North America with respect to the non-contagious nature of the yellow fever, would take place in Europe with respect to typhus," p. 160.

Again Dr. B. tells us, that—

"Dr. Armstrong was convinced that malaria was at least their primitive remote cause, and that the typhus of Great Britain could not be considered essentially a contagious disease; an opinion which was so much opposed to the general sentiments of the profession in this country, that in the zeal of his own convictions, he speaks of the proofs of its origin in malaria as a discovery. But other observers," Dr. B. very properly adds, "had long before come to the same conclusion, though no one had attempted to establish them on such definite grounds," p. 299.

We easily excuse the concluding salvo to the manes of so dear a friend. But had Dr. Armstrong lived in America, he could not have found it so easy to persuade himself that he had found a new thing in discovering proofs of the non-contagious character of typhus fever; especially using the term in his "comprehensive signification" as embracing a great variety of forms of fever. In regard to true typhus, in the restricted, and, as we think, the proper sense of the term, he would have found few physicians, so far as our observation extends, who would have agreed with the profession in London, as he represents it, in believing that it originates "*solely* in human contagion;" although he might find those who suspect or believe the existence of some modified contagious influence, coöperating under certain circumstances with other causes in the propagation of the disease. But in respect to most of the fevers which Dr. Armstrong includes under the designation of typhus, the question has long been regarded by a large proportion of the profession in this country as so fully settled, that a further discussion of it is less called for here, as we have already intimated, for the sake of giving more light upon it, than as furnishing a good summary of the evidence which has determined it.

Considering how large a part of our medical literature is received from Europe, it is not a little remarkable that the opinions of physi-

cians on this subject here should be so much in advance of those of the old world. Indeed, we hardly know which is the most surprising, that the pupil should thus have outstripped the master; or that, on the other hand, the physicians of Europe should be su slow at receiving any opinion or established observations from the new world. It were not to be expected that the great body of English, Scotch, and Irish physicians, to whom, from the community of language, this remark chiefly applies, should know much of the state of medical opinions at such a distance from them, or should condescend to notice, if they had heard of, what comes from so youthful a country. But from the public medical journals of Great Britain, and especially from the public teachers of medicine, it might have been hoped that they would look out for true knowledge wheresoever it is to be found.

That Dr. Boott is not liable to the imputation of neglecting American medical literature, is sufficiently obvious from the very nature of the publication before us. With him the question of the contagion f fever is to be settled by a wider range of observation than falls to the lot of any single individual, however extensive his opportunities may be; and he naturally turns his eyes first to this country, to which he gives the credit of having made greater advances towards settling it than have been made elsewhere. If his volume shall have the effect o furnish to English physicians a just estimate of the state of medical science in this country, he will have rendered an important service to the profession in his own country, while American physicians may well acknowledge their obligations to him for such a service.

Having gone through with his elucidation of Dr. Armstrong's views of the nature and the symptoms and treatment of fever, Dr. Boott goes on to a consideration of the remote cause of fever. Here he begins with a statement and defence of Dr. Armstrong's opinion founded upon the assumption, which we have already noticed, that typhus has a common origin, and common character with intermittent and remittent fever. But he soon breaks loose from the trammels of supporting a system, and considers the question independently as the evidence is presented by the history and phenomena of each disease.

He begins with yellow fever; under which name Dr. A. believed that three different affections had been included, viz. acute hepatitis; the inflammatory endemic of the West Indies, a *common* fever arising from heat acting on unseasoned constitutions; and the epidemic bilious remittent, the offspring of marsh effluvium, and therefore a *specific* fever. Having considered somewhat at length the character of the two last varieties of yellow fever, Dr. B. proceeds to the inquiry

whether there is a fourth variety to be found in the Boulam fever, so earnestly contended for by Dr. Chisholm, and the other advocates for the contagion of this disease. The supposed origin of this fever he examines in detail, and fully establishes the conclusinn that there is no ground for regarding it as a distinct disease. Our limits will not permit, neither is it necessary that we should follow our author through his examination of the evidence in support of this result, exhibited during the prevalence of yellow fever at different times in Philadelphia, Wilmington, Port of Spain, Baltimore, and other places. Suffice it to say, that with a sufficiency of detail, he has given a condensed, impartial, and to our minds a conclusive view of the subject.

This view is greatly strengthened by the survey which he next takes of the fevers that have appeared at different times in various parts of the United States. The leading object of this survey is to show the affinity which our author, in common with his distinguished friend, believes to exist between intermittent and remittent fever, and typhus.

"Dr. Bancroft," he says, "has asserted that typhus is not to be found in tropical countries; and no one will dispute the assertion, if he means a fever characterized by all the symptoms of the typhus of Great Britain; but that a specific continued fever actually exists in either India, the product of malaria, differing in no other essential respect from our common endemic than might be expected from the difference of climate, is proved by the observations of several authors. A similar fever is also frequently mentioned in America, even in places where yellow fever exists; but its occurrence is most common in the midland and northern states, north of lat. 39° or 40°, where the temperature is on the average too low to give full development to yellow fever, at least in its epidemic form," p. 301.

We have on a former occasion* remarked somewhat at length on the unsatisfactory nature of the inference, that even typhus is every where the same disease, because it has in many respects similar habits, and obeys similar laws; and we should now find it easy to exhibit what seem to us insuperable difficulties in the way of regarding the several fevers here considered, as identical in any proper sense, or in any such sense as will be useful, or even harmless either in theory or practice. But our limits forbid a prolonged discussion, and we have little inclination to contend on mere points of opinion, where the facts are so faithfully exhibited. As a matter of analogy, though in our view not as decisive authority, all that is here said is fairly brought into the argument. Independently of all considerations of theory, it is a highly interesting and valuable survey.

* Review of Smith and Tweedie on Fever.

Beginning with the southern states, Dr. B. first brings together accounts of a great number of fevers as they prevailed at different times in those latitudes in which yellow fever most frequently prevails. These fevers were obviously endemic in their origin, and varied in the different cases, from a tolerably mild intermittent to malignant remittent, and continued fever; and in many instances the different forms interchanged, as the prevalence of the disease increased or declined. We cannot accompany our author through this survey, nor examine the conclusions to which he arrives. But our national vanity, (it may be,) will not suffer us to omit the following flattering testimony to the observations of American physicians.

"I must again repeat, that I know of no body of facts more instructive in the whole compass of medical literature, than those contained in the reports of medical men scattered through the states of North America; men, who, however imperfect their early education might have been, as compared with those who had also the advantages of the elaborate instruction of the European schools; thrown as they necessarily were upon their own resources, in a country fertile in all the varieties of fever, and obliged to discharge *all* the duties of the profession, may be said ultimately to have been taught in the great school of nature, where the observation of her phenomena led to the best practical information, unfettered by those systems and creeds which hold such influence over the minds of men in a narrower field of observation, and limited, as in this country, to the practice of one particular branch of the profession. A comparison of the medical literature of America from the time that Rush's views began to be widely diffused, with that of this country up to the publication of Mills' work on the efficacy of blood-letting in fever, would, I am confident, reflect honour on America," p. 391.

"Rush had thirty years before inculcated what are considered at the present day sounder views of disease; and his influence in America was equal to that of the great Edinburgh professor in this country. 'We remember, (says a well-known journalist, in 1809,) the horror and incredulity that was excited some years ago by the evacuating system of Dr. Rush, and of some West India practitioners in yellow fever; and how a celebrated German professor of the stimulating school shrugged up his shoulders, and prognosticated the certain death of a patient in our infirmary labouring under typhus, when he heard his physician prescribe some necessary evacuations. But the practice of Dr. Rush was successful, and our condemned patient speedily recovered.*'" p. 393.

The survey which our author next takes of the fevers of the "midland" states, occupies somewhat more than a hundred pages of his volume. It is an able summary of the accounts of yellow fever at Wilmington, and at Philadelphia; the latter chiefly from Dr. Rush. Dr. Rush's own most interesting account of this portion of our medical history, is too familiarly known to our readers to require from us

* Edinburgh Medical and Surgical Journal, Vol. 85.

any notice of it. Dr. B. then turns to the fevers of the city and state of New York.

"Among which," he says, "we shall find examples of yellow and typhus fevers; the last appearing, as at Wilmington, in Delaware, in those years when the state of the atmosphere was not such as to give rise to the former modification of fever."

"I have already remarked," he adds, "that if malaria be the primary origin of typhus, as well as of the common remittent and yellow fever, and that if a continuance of high heat be essential to the latter, we might expect, in a country of such vast extent as North America, to find, as we examined the character of fever progressively from the southern to the northern portions of the continent, that yellow fever, which is annually endemic at Vera Cruz, and New Orleans, would be represented by some other form in those places where the mean temperature from latitude would be too low to give that irritability to the animal organs, or that concentration and intensity to malaria, which are considered essential to the full development of it. It will be seen that this change of type actually occurs in different years; and that New York, which is situated in latitude 40° 42′ N., is the last place in this progressive inquiry from south to north in which we meet with any frequent examples of severe and extensive epidemics of yellow fever," p. 513.

The volume closes with a similar but concise view of the fevers of the eastern states. In this we find little notice of the typhus fever, strictly so called, which occurs to a greater or less extent almost every year throughout New England. This we believe is not to be attributed to design or inattention on the part of the author, so much as to a want of the necessary authorities. It would have interested us greatly to have seen a faithful and able comparison, such as he would have given us, of the typhus of New England with that of Great Britain. But we freely confess we should ourselves have been somewhat at a loss to refer him to the materials on our part, necessary for such a comparison; and we cannot therefore complain that it is not given us. The truth is, typhus in New England rarely occurs, except in sporadic cases, and attracts so little of general attention, that very little seems to have been written respecting it. No one physician sees so much of it, as to feel authorized to write about it, in the expectation of instructing his neighbours, and all, or nearly all, see enough to lead them to forget that it may be an object of curiosity and interest to the medical public.

In concluding our notice of this work, we are not without apprehensions that we have not sufficiently expressed our high sense of its value and interest. Although we have not been able to enter into all the feelings of admiration and respect towards the public character and attainments of Dr. Armstrong, with which an ardent friendship

had inspired the author, we would feign hope that we have done justice to the excellence of those feelings, and to the personal worth which gave rise to them. And we have wholly failed in giving expression to our opinions, if we have not made it appear that his account of the fevers of this country is a work for which every American physician may well feel grateful to him.

We look for the appearance of the second volume with eagerness. The promised account of European fevers, if written with equal faithfulness and ability, will furnish us with a highly interesting and valuable piece of medical history. We are glad to learn that as soon as that volume is received, the whole work will be immediately issued from the press in this country.

E. H.

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Art. XVI. *Nouveau Système de Chimie Organique, fondé sur des methodes nouvelles d'Observation*, par F. V. Raspail. Accompagné de douze planches gravées, dont six coloriées. Paris, 1833, pp. 576. 8vo.

THE close and intimate connexion existing between the sciences and their dependence on each other for an explanation of the phenomena peculiar to each, becomes more and more evident, as we advance in knowledge. Even chemistry, whose limits were formerly considered as perfectly defined, has within a few years assumed an entirely new aspect, and her votaries, instead of relying exclusively on crucibles and reägents as the only legitimate instruments of research, have called to their aid the apparatus and laws of other departments of learning, and have thus been enabled to extend the domain of their art in a wonderful and unexpected manner.

This union of means has been strikingly successful in our researches into the ultimate composition of bodies, and has afforded results whose truths can be verified by the strictest rules of mathematical calculation. By the theory of atoms and that of definite proportions, we are enabled to ascertain with certainty the primary constituents of inorganic substances, and the rules which govern and modify their combinations, so as to be able in a vast number of instances to imitate nature, by forming these combinations at will. But when we attempt to apply these rules to the explanation of the phenomena of organized beings, we find our resources fail, and are obliged to confess the futility of our means. For although animal and vegetable substances are composed of a very small number of